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New vistas in the study of memory

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This book presents important new findings and new ideas about remembering: what people remember, why they remember it, and how we might best describe the processes involved. These issues have been considered before, most notably by Sir Frederick Bartlett in the 1932 book from which our title is derived. Arguing that standard laboratory methods did not do justice to what he called the subject's "effort after meaning," Bartlett introduced both new procedures (recall of meaningful stories) and new theoretical concepts (e.g., "schema") into the study of memory. Although the influence of his work was slow to develop at first, its importance is now widely acknowledged: Story recall has become a standard laboratory technique, and "schema" a standard theoretical idea. No longer revolutionary, Bartlett's insights have been effectively assimilated into the traditional psychology of memory.

In the mid-1970s I undertook a general critique of contemporary mental-model-oriented cognitive psychology, including the study of memory (see *Cognition and Reality* [Neisser, 1976]). In the hope of stimulating a serious reconsideration of the prevailing assumptions in memory research, I presented a paper sharply critical of traditional approaches at the Cardiff conference on "Practical Aspects of Memory" in 1978. After nearly a century of research, psychologists still knew almost nothing about recall of early childhood experiences, about memorizing songs and poems, about memory for places, faces, and names, about the fate of knowledge acquired in school, about oral history, about testimony, about prospective remembering, or even about individual differences. I summarized these omissions in a simple rule: "If X is an interesting or socially significant aspect of memory, then psychologists have hardly ever studied X" (Neisser, 1978, p. 4).

I developed the argument somewhat further in a book of readings called *Memory Observed: Remembering in Natural Contexts* (Neisser, 1982). The book presented some 40 examples of research from what I called "the low road" in memory research, to distinguish it from "the high road" of standard laboratory procedures. Following the high road had

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not been fruitless: Valid empirical generalizations had been established, discoveries made, sophisticated models and theories proposed. Nevertheless, the overall result left much to be desired. The empirical generalizations were often rather obvious (e.g., the role of similarity in interference), the discoveries were intriguing but hard to intepret (e.g., the dependence of memory search time on the number of alternatives), and the models, which never applied outside of narrowly defined experimental contexts, were invariably soon disconfirmed. Although those who followed the low road had even fewer achievements to their credit, there was reason to believe that their route might open up new vistas. The principal goals of *Memory Observed* were to illustrate the possibilities of naturalistic memory research and encourage people to do more of it.

As things turned out, very little encouragement was needed. The past 10 years have witnessed a surprising surge of interest in the ecological approach to memory, and naturalistic studies have begun to appear in quantity. The principle that I enunciated in 1978 now almost applies in reverse: If X is an interesting or socially significant aspect of memory, some psychologist is probably trying to study it at this very moment! To some extent, this trend is part of a generally growing interest in the ecological approach to cognition, an approach that has implications ranging from the study of direct perception (Gibson, 1979) to concepts and conceptual development (Neisser, 1987). But that is only a small part of the story; most of those who are now engaged in naturalistic memory research came to it without any strong theoretical commitments. They have been primarily interested in the phenomena themselves – in autobiographical memory (Rubin, 1986), in school learning (Bahrick, 1984), in prospective memory and absentmindedness (Harris & Morris, 1984), in legal testimony (Wells & Loftus, 1984), in remembering events and places and people and whatever else there is to be remembered (Gruneberg, Morris, & Sykes, in press).

As always, success brings new responsibilities in its wake. Now that we have all these new facts, what are we to make of them? Are they adequately explained by principles derived from traditional theories of memory, or are entirely new concepts needed? If so, what are those concepts? It is no longer enough to denounce the old laboratory methods and call for more ecologically oriented studies; we have now to examine the findings of those studies and try to understand them. And in doing so we must not make the mistake of supposing that the "traditional" psychology of memory has simply been standing still, waiting for the ecological approach to come along. Since the mid-1970s, the laboratory-based study of remembering has undergone what amounts to a revolution of its own. Tulving's distinction between semantic and episodic memory, the postulation of "schemata" for everything from stories to selves, the

research on scripts and event representations, the rush of new findings on memory development in children – all these are signs of renewed vigor and creativity in the field that I criticized so sharply a decade ago. In chapter 14 of this book, I try to interpret these new developments in ecological terms: In effect, they expand our definition of what kinds of real things exist to be remembered. Partly for that reason, I believe that future relations between ecological and traditional studies are more likely to be complementary than antagonistic. In any case, it seems that the time may be ripe for another reconsideration of remembering.

This book is the product of the second Emory Cognition Project Conference, held in Atlanta on October 10–12, 1985. Although the official focus of the conference was "Ecological Approaches to the Study of Memory," the invited speakers did not fit any single mold. Some of them had naturalistic findings to report, some had theoretical ideas to develop, and some had been doing laboratory research that was simply too important to ignore. Their presentations and interactions left us with a sense that a new psychology of memory is beginning to take shape – one that will eventually yield theoretically consistent interpretations of both laboratory paradigms and naturalistic phenomena. We do not have that psychology yet, but we are moving.

A brief overview of the chapters ahead may be helpful at this point. In chapter 2, my coeditor **Eugene Winograd** analyzes the convergence of ecological and traditional research findings in some detail, drawing not only on the work of contributors to this volume but also on other well-established phenomena. He notes that both research traditions are now concerned with issues of verifiability, that both have developed a new interest in the shape of the forgetting function, that both have recognized the critical importance of the distinctiveness of cues. Moreover, both can be considered as the logical successors of "functionalism" – a significant historical tradition that has consistently combined rejection of unnecessary mental hypothesis with an emphasis on adaptation and purpose.

In chapter 3, **William F. Brewer** reports a pair of extraordinary studies of memory for very ordinary events. His subjects carried "beepers" that went off randomly; whenever the beepers sounded, they wrote down where they were, what they were doing, and what they were thinking. Weeks or months later, Brewer tested their memories by recognition (Study 1) or cued recall (Study 2). His findings are rich and important. Memory for these unselected events was (predictably) poorer than in diary studies, in which subjects try to record "the most important event of the day," but forgetting proceeded very slowly, and there were almost *no* intrusions or false recalls. In the many instances when activity and thought were not closely coordinated, memory for the former was better than for the latter. Recall of actions depends most strongly on their

uniqueness; recall of thoughts depends most strongly on their degree of excitement. Although many of these findings can be taken as confirmations of traditional principles, application of those principles to such data demands a detailed understanding of the actual lives of the subjects. Brewer is concerned with the theoretical interpretation of his results; for example, he takes the very low error rate to mean that memory is rather less "reconstructive" than he (and I!) used to believe. He also reports a new and interesting finding about the experience of remembering itself: Reports of vivid imagery at the time of test were associated with more complete recall. (But not necessarily with more *accurate* recall, it seems to me. In a study where subjects never make errors, it is logically impossible to establish correlations with accuracy.)

In the data reported by **Craig R. Barclay** and **Peggy A. DeCooke** (chapter 4), the to-be-remembered events were recorded by the subjects at the end of each day. The subsequent recognition tests included "foil" events that had not actually happened, but had been rewritten from the subjects' records. As might be expected on the basis of traditional principles, the probability of being deceived by such a foil depends on exactly what was changed in it and on the similarity between the altered item and the original. Because one of their subjects sustained a tragic personal loss during the time that the study was in progress, Barclay and DeCooke were also able to compare recall of a very important and stressful experience with memory for more mundane daily events. Overall, the stressful experience was remembered much better.

In a thoughtful commentary, **Robert N. McCauley** (chapter 5) discusses the notion of "memory for thoughts" in the light of modern philosophical concerns about reference and meaning. He also points to various difficulties of design and interpretation in the studies reported by Brewer and by Barclay and DeCooke. In a particularly interesting argument, McCauley disputes Brewer's claim that a low error rate is evidence against "reconstructive" theories of memory. Perhaps, he suggests, the subjects of the beeper experiments were just accurate in their reconstructions!

Larry L. Jacoby, who explicitly identifies himself as a functionalist, begins chapter 6 with a vigorous defense of traditional laboratory methods. He then shows in a series of ingenious experiments that past experiences that the subject does not recall – and cannot even identify as familiar – may nevertheless exert specific and long-lasting effects. This is not because structurally distinct memory systems are involved: Jacoby rejects structural hypotheses on principle. He suggests that the feeling of familiarity is itself the result of a process of attribution, vulnerable to error under certain conditions just as other attributions are. More generally, he notes that a detailed examination of the relation between encod-

ing conditions and retrieval conditions often makes it unnecessary to postulate generalized mental representations, complex cognitive systems, and other hypothetical structures. Jacoby's reluctance to postulate mental structures in memory reminds me forcibly of James J. Gibson's approach to perception, though he obviously does not share Gibson's antipathy to traditional laboratory methods.

Harry P. Bahrick and **Elizabeth Phelps,** who also call themselves functionalists, present a new and elegant study of long-term semantic memory in chapter 7. Their study differs from Bahrick's earlier work on long-term retention (of Spanish learned at school, in Bahrick [1984]; of the layout of a town, in Bahrick [1983]; of high school classmates, in Bahrick, Bahrick, & Wittlinger [1975]) by introducing a specific experimental intervention. They begin by identifying items of information that the subject may once have known but now no longer remembers: bits of general knowledge, vocabulary words from a foreign language, and the names of pictured, once-famous individuals. Subjects are then given minimal "refresher" trials on some of this material (one correct trial per item). These refreshers have powerful effects, even on items that subjects did not think they knew at all when the experiment began. The relearning of such items is very much faster than the learning of entirely new (but comparable) material by control subjects. This is not only a novel finding but also an encouraging one: Evidently we all know a great deal more than we think we do. Much of that knowledge may not be difficult to reinstate with the procedures developed by Bahrick and Phelps.

In chapter 8, **Lawrence W. Barsalou** describes an approach to autobiographical memory that was originally stimulated by work in artificial intelligence (AI). He begins with a naturalistic study: People were simply asked to "tell me about events you were involved in this past summer." Surprisingly, their responses did not consist primarily of reports of specific events or occasions. Subjects usually recalled their summers in terms of generic (repeated) events, extended (noncontinuous) events, and comments. Barsalou considers these results in terms of various possible modes of mental organization – by events, by participants, by locations, and by times. A series of experiments with artificial materials then suggests that people can organize remembered experiences in a variety of different ways, with "global sequences of chronologically ordered extended events" being the most fundamental. Barsalou uses these findings as the basis of a rather elaborate theory: Autobiographical memory is structured in terms of "extended-event time lines" that are themselves hierarchically (perhaps spatially) organized. Remembered events involve sets of particular "exemplars" linked by various kinds of conceptual relations; generic mental representations also play an important role.

Barsalou's AI-oriented approach does not fit comfortably into either an ecological or a traditional memory niche, but his ability to address the issues raised by theorists of many different persuasions gives his work a special interest.

Although "cognition" and "cognitive development" are sometimes treated as separate fields of study, they are really just two ways of thinking about the same set of problems. In accord with this principle, about half the chapters in the first Emory Cognition Symposium (*Concepts and Conceptual Development* [Neisser, 1987]) were devoted to developmental issues. The third symposium volume (*What Young Children Remember and Why* [to be edited by Fivush & Hudson]) will deal entirely with the development of memory. In the present volume, however, there are only two developmental contributions: Nelson's chapter 9 and Fivush's subsequent commentary. **Katherine Nelson** offers nothing less than a functionally based account of the development of memory over the first few years of life – the most detailed and empirically justified such account that has yet been undertaken. As might be expected, she relies heavily on the large body of research on young children's memory that she and her collaborators have carried out so impressively in the last several years. In addition, she presents a set of new data based on the recorded presleep monologues of a single subject, Emily, who was 21 months old at the beginning of the study and 36 months at the end. Taken together, these findings allow Nelson to trace the early development of memory for events, as well as memory for scripts and routines, to examine the relations among remembering, fantasizing, and anticipating, and to speculate about the origins of genuinely autobiographical memory somewhat later in childhood.

Robyn Fivush's commentary in chapter 10 focuses on the role of memory in self-definition. Our notions of who we are depend heavily on what we remember about ourselves – not so much on specific past episodes, but on typical or extended sequences of events. Fivush finds striking structural similarities between Emily's later monologues and the responses of Barsalou's adult subjects recalling their summer vacations: Both are characterized by hierarchically organized routines in which summarized events are embedded. With age and development, the embedding events start to involve longer and longer time periods. Eventually they begin to form what amounts to an extended life history, and a genuinely autobiographical memory structure takes shape.

One of my own favorite examples of naturalistic research in memory is Albert Lord's study of ballad singers in Yugoslavia (Neisser, 1982, pp. 243–257). In chapter 11, **Wanda T. Wallace** and **David C. Rubin** report what amounts to a carefully controlled, quantitatively analyzed modern American replication of Lord's work. Their material is a folk ballad –

"The Wreck of the Old 97" – that is still sung in the hills on North Carolina. Wallace and Rubin used an elegant combination of field and laboratory techniques. They recorded the "Wreck" as it was actually sung by five different ballad singers, each on two different occasions; they also presented naive college-student subjects with various specially modified versions of the song and asked them to learn it. The data show in detail how various kinds of constraints – metric, poetic, and meaningful – inter-act to determine what is recalled. They summarize their findings as follows: "The memory for the ballad is not the exact song, nor is it a collection of words; rather, it is a collection of rules and constraints. . . . We have not only a schema for gist but also a schema for poetics, rhythm, imagery, and music. Together these schemata, and possibly others, con-strain recall to the extent that it almost appears rote or verbatim." It seems to me that this work offers a striking example of the implications of ecological research for a domain – rote memory – that has usually been studied only in the laboratory.

Donald P. Spence, who writes about passive memories in chapter 12, is the author of *Narrative Truth and Historical Truth* (1982). Whereas that book was concerned primarily with recall during psychoanalysis, his chap-ter in this book deals (albeit speculatively) with several kinds of everyday memory experiences. In some situations a vivid memory may come to us unbidden, fleetingly, stimulated by some nearly unnoticed and often unreportable aspect of the "enabling context." Later on, such a memory may be incorporated into our recall of that context itself, lending it an air of fantasy and unreality. A related phenomenon is what Spence calls "repetitive remembering," where a particular memory comes to mind over and over again in a particular context, although it has no practical significance whatever. Spence suggests that repetitive memories often serve a defensive function, preventing us from remembering something that would be much less acceptable. He also notes that the "repetition" may be more apparent than real; repetitive memories may be especially subject to the systematic schema-based changes that Bartlett called to our attention long ago. Spence concludes with an example of one of his own memories, now some 15 years old, recalled on two different occasions. The second recall was far richer, more detailed, and more affect-laden than the first. Repetition can apparently lead to elaboration as easily as to schematization; it all depends, Spence believes, on the enabling context.

I have occasionally suggested (e.g., Neisser, 1985) that one way to begin a genuinely ecological approach to the study of memory would be with a preliminary list of the kinds of things that people actually remem-ber. In chapter 13, **Steen F. Larsen** introduces a class of memoria that has been almost universally overlooked by theorists of memory, myself included: reported events of which we have only secondhand knowl-

edge. This large category of memory includes events we have heard about from friends, read about in the newspaper, or watched on television. Such memories are neither "episodic" nor "semantic": Although not referenced to the self, they may be quite specific to a particular time and place. Larsen offers a detailed analysis of the characteristics of memory for reported events and contrasts it with memory of other kinds. For example, there is usually a dual structure that includes a "reception event" as well as the reported event itself. Sometimes the reception event can become a target for memory in its own right, as in Brown and Kulik's (1977) study of "flashbulb memories," but usually it is of little interest and soon forgotten. (This may be an individual-difference variable. I myself find it very hard to remember who told me things, even quite recently, but other people do not seem to have the same problem.) Larsen also notes that events as reported by the news media have a narrative structure of their own, quite different from the structure of more frequently studied memory materials such as stories or autobiographical episodes. How could we have failed to notice them for so long?

My own chapter 14 makes three more or less independent arguments. The first is an ecologically oriented analysis of what the traditionally oriented psychology of memory has accomplished in recent years. On this interpretation (not the only possible one, of course) the biggest single change has been the introduction of several new classes of "memoria"–things that people admittedly remember. Where psychologists used to study only memory for lists and list items, they now study memory for facts, stories, familiar routines, academic materials, geographical layouts, and personally experienced events–an enormous and highly significant change. Turning to the last of these categories, the second part of my chapter proposes a hierarchical analysis of the memoria of autobiographical memory itself. Real events always have a nested structure, with events of smaller scope embedded in more comprehensive ones. We apprehend, understand, and apparently remember events in just this way. I therefore suggest that the structure of autobiographical memory itself–the way the information is stored "in the head"–may be hierarchical in some corresponding sense. Finally, in a third section, I explore the (rather farfetched) conjecture that human autobiographical memory depends heavily on the mammalian spatial orienting system. The behavioral and neuropsychological parallels between the two systems are at least intriguing, and both seem to have similarly nested hierarchical structures.

In a final commentary (chapter 15), **David C. Rubin** is sharply critical of my second claim. To speak of "nested" structure in memory, he suggests, is to make a structure out of a process. It is one thing (and usually useful) to describe the environment at some appropriate level of analy-

sis; it is another (and usually unfortunate) thing to copy that description into the mind. What people acquire as they journey through life are not structures but *skills:* capacities to act in certain adaptive ways. Skills may require stored information, but they do not require stored environment-mimicking structures.

I am grateful for Rubin's eloquent argument. Having argued for the primacy of skill over structure myself in other contexts (Spelke, Hirst, & Neisser, 1976; Neisser, 1983), I am not inclined to dispute it here. Admittedly I find it harder to dispense with structure where memory is concerned, but that is no reason not to try. Perhaps it would be better to say only this: If we want to think of the information in memory as having an intrinsic structure, that structure will turn out to be at least as complex as the hierarchies I have tried to describe. The issue is certainly still open. Indeed, a great many issues in the study of memory – ecological or traditional – are presently open; that is why this is a good time to reconsider them. The ecological and traditional approaches to memory are both making substantial progress, perhaps even in the same direction.

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